President Everett
with respects of
the author

# REVIEW

OF

# HERSCHEL'S OUTLINES OF ASTRONOMY.

FROM THE CHRISTIAN EXAMINER FOR SEPTEMBER, 1849.

CAMBRIDGE:
METCALF AND COMPANY,
PRINTERS TO THE UNIVERSITY.
1849.

# REVIEW

OF

# HERSCHEL'S OUTLINES OF ASTRONOMY.

---

FROM THE CHRISTIAN EXAMINER FOR SEPTEMBER, 1849.

---

CAMBRIDGE:

METCALF AND COMPANY,

PRINTERS TO THE UNIVERSITY.

1849.

# REVIEW.

*Outlines of Astronomy.* By Sir John F. W. Herschel. London. 1849. pp. 661.

It is unfortunately too often the case, that those who have attained to high culture in any department of knowledge find it irksome to clothe their thoughts in a popular form, and to communicate in ordinary language with the public. This arises in part from the difficulty of expressing themselves in common words with that nicety to which they are accustomed and which their habit of mind demands; but still more from the fact, that the talents and taste which stimulate to original researches are seldom found combined with the rhetorical acquirements which are necessary to fix the attention of differently constituted minds. For the teacher, a certain diffuseness is indispensable. His vocation requires him, as Fichte says, "not to communicate his idea as the author does, abstractly and in the one perfect conception under which it presents itself to his own mind, — but he must mould, express, and clothe it in an endless variety of forms, so as to bring it home, under some one of these garbs, to those by whose present state of culture he must be guided in the exercise of his calling. And, above all, he must possess the creative or artistic talent of the scholar."

In consequence of this repugnance to diffuse knowledge on the part of those whose calling it is to increase it, the second class of scholars has arisen, — the class who receive from the original discoverer and distribute to their fellow-men. Their profession is in itself a noble one, because without it the first would labor without benefit to their race; but, in consequence of their lower degree of culture, much error becomes intermixed with the knowledge they diffuse, in the very process of distribution.

When, therefore, a scientist, of high attainments, an original investigator, devotes himself with earnestness to the work of adapting to popular comprehension his own hard-earned knowledge, his labors are entitled to the most respectful consideration, and, if they answer their purpose, will be sure of the gratitude of the community.

It was with high gratification that we received, some time since, the announcement of a new popular work on astronomy, by Sir John Herschel. His reputation for versatility of talent and elegance of scholarship, and his past labors in astronomy and photography, have gained for him an enviable position; and, unlike most men of equal eminence, he has striven to diffuse the knowledge which he has labored to increase. If there be any one from whom the public would be warranted in anticipating a thorough, accurate, and elegant popular work on astronomy, in the English language, it is Sir John Herschel. The "Discourse on the Study of Natural Philosophy" was published fifteen or twenty years ago; and the author, occupying as he does a distinguished position among European astronomers, and possessing the authority of a doubly illustrious name, has unquestionably exerted, through this book, a highly beneficial influence upon the public mind.

In April last, the work appeared, whose title stands at the head of this article, — a work which professes to be an extension of the "Treatise on Astronomy," formerly published. The author says that the "Treatise" has been revised and remodeled, and much new matter introduced; that the parts relating to the lunar and planetary perturbations have been rewritten upon a far more matured and comprehensive plan; and that those on sidereal and nebular astronomy have been brought up to the present state of our knowledge.

We have carefully read the book, and do not hesitate to say that we are disappointed. The mechanical execution is beautiful; the text is comparatively free from typographical errors; the plates and maps are finely engraved; and the appearance of the volume must make an agreeable impression. But throughout the work, or, at least, throughout the new parts of it, the indications of inaccuracy are too numerous to allow us to place implicit confidence in any statement before verifying it.

Of the style we do not propose to speak. It may suffice to say that it is very unequal. While some passages are exquisitely beautiful and interesting, or thrillingly eloquent, others are so obscure as to be almost unintelligible. A single example will illustrate our meaning: —

"Now, though we cannot see the path of a star in the heavens, we can wait till the star itself crosses the field of view, and seize the moment of its passage to place the intersection of its wires so that the star shall traverse it; by which, when the telescope is well clamped, we equally well secure the position of its diurnal circle as if we continued to see it ever so long." — p. 99.

Some of the expressions rivet the reader's attention, and compel his admiration by their felicity and singular aptitude, while others seem, at least to an American ear, almost pedantic, as when the author speaks (p. 388) of "the *orthogonal* [? perpendicular] component of the disturbing force," or says (p. 405) that it is impossible to give any idea of "the analytical conduct" of Lagrange. We notice that Sir John uses Dr. Whewell's word "thermotics." Why not "thermics"? And, if the word *optics*, in the sense of the science of light, be discarded, "photics" would seem more convenient than "photology," and quite as conformable to established analogy.

The introduction is reprinted with but slight change from the former work. It is a beautiful chapter, clear and concise, informing beginners in astronomy what they have a right to expect from an elementary work on this science.

"Its utmost pretension," says Herschel, "is to place them on the threshold of this particular wing of the temple of Science, or rather on an eminence exterior to it, whence they may obtain something like a general notion of its structure; or, at most, to give those, who may wish to enter, a ground-plan of its accesses, and put them in possession of the password. Admission to its sanctuary, and to the privileges and feelings of a votary, is only to be gained by one means, — *sound and sufficient knowledge of mathematics, the great instrument of all exact inquiry, without which no man can ever make such advances in this or any other of the higher departments of science, as can entitle him to form an independent opinion on any subject of discussion within their range.* It is not without an effort that those who possess this knowledge can communicate on such subjects with those who do not, and adapt their language and their illustrations to the necessities of such an intercourse. Propositions which to the one are almost identical are theorems of import and difficulty to the other; nor is their evidence presented in the same way to the mind of each."

The book is divided into four parts. The first comprises more than half the volume, and treats of spherical astronomy, astronomical instruments, and the bodies of our solar system; the second is devoted to the theory of planetary perturbations; the third is on sidereal astronomy; and in the last, which consists of but a single chapter, is a description of the several ways of keeping account of time, and of the different calendars.

The first few chapters are occupied with general ideas and elementary conceptions, terminology, and the like. Although these would naturally demand a place at the commencement of a popular work, and although they are elaborately given, yet we much doubt whether, as they stand, they will be of any service to beginners, unless perhaps these chapters may answer as a dictionary of technical terms. We say this on account of the obscurity which they would present to the class of readers for whom the book is designed. Statements of simple propositions are made in technical language, and enveloped with a shroud of symbolic letters; which, however clear to those accustomed to mathematical studies, are still in no wise attractive to the general reader. For instance, in the note to p. 55, speaking of the relative motion of two bodies, he says: —

"If two bodies, A and B, be in motion independently of each other, the motion which B, seen from A, would appear to have if A were at rest, is the same with that which it would appear to have, A being in motion, if, in addition to its own motion, a motion equal to A's, and in the same direction, were communicated to it."

This proposition seems to us indeed to require "more thought for its clear apprehension than can perhaps be expected from a beginner," more, indeed, than should be demanded of any one for the comprehension of so simple an idea. We believe that its meaning is merely, — that the real motion of a body (which is seen from another moving one) is the resultant of its apparent motion and that of the observer.

An anecdote is related upon page 20, in connection with the remarks on the "dip of the horizon." "The history of aëronautic adventure" is said to "afford a curious illustration" of this principle. A celebrated aëronaut, by the name of Sadler, descended in his balloon nearly to the surface of the sea, after sunset; but, throwing out his ballast, suddenly rose again to a great height, and enjoyed "the whole phenomenon of a western sunrise." On descending again, he saw the sun set a second time. It is somewhat remarkable, that, in the course of his long Atlantic voyages, the author had never availed himself of a means of enjoying the same curious illustration, without any expenditure of gas. The masts and rigging of a ship furnish all the necessary apparatus, as every sailor, and almost every passenger, knows. It is by no means an unusual thing for an observer at the mast-head, or even at the crosstrees, to witness a sunrise, and then, descending rapidly, enjoy what the author would call the whole phenomenon of an eastern sunset.

A page or two farther on, the height of the atmosphere and of clouds is discussed. Sir John there states that "it seems probable, from many indications, that the greatest height at which visible clouds *ever exist* does not exceed ten miles; at which height the density of the air is about an eighth part of what it is at the level of the sea." In a report to the French Academy, concerning the voyage of the frigate Venus in the Atlantic Ocean and South Sea, the commander, Admiral Du Petit Thouars, names as the maximum of the observed height of clouds, fourteen hundred meters.* Kaemtz, however, in his Treatise on Meteorology, (i. 384,) states, that, on one occasion, a cloud was observed at the height of sixty-five hundred meters. This would give a maximum height of about four miles. We cannot, therefore, but cordially agree with Herschel that their greatest height probably never does exceed ten miles.

It is an interesting question at what height the specific gravity of the atmosphere would permit visible vapor to remain suspended. The density of air at the height of ten miles would correspond to a barometric pressure of one hundred and two millimeters, — about four inches.

The chapter upon astronomical instruments and observations will probably be useful to the beginner; although, as we should expect, English instruments are described rather than continental ones, and the student is referred to Dr. Pearson's Astronomy. The standard of precision is a corresponding one; — as when we read (on the same page) that "in good transit observations, an error of *two or three tenths of a second of time* in the moment of a star's culmination is the utmost which need be apprehended, exclusive of the error of the clock." Should this meet the eye of any of the German or Russian astronomers, they will be indeed amazed at the degree of precision which may be obtained!

We were somewhat surprised on reading the note at the bottom of the 103d page. Sir John Herschel there says: —

* A mile is a little more than 1609 meters.

"By a peculiar and delicate manipulation and management of the setting, bisection, and reading off of the circle, aided by the use of a movable horizontal micrometric wire in the focus of the object-glass, it is found practicable to observe a slow-moving star (as the pole-star) *on one and the same night*, both by reflection and direct vision, sufficiently near to either culmination to give the horizontal point, without risking the change of refraction in twenty-four hours; so that this source of error is completely eliminated."

Although the author seems to have been unaware of many of the refinements introduced into the continental observations, it is astonishing that he should not have known that it has been for many years the usage at Greenwich to observe not merely the slow-moving, but also the equatorial stars, at the same transit, both by reflection and by direct vision; — the star being directly observed over one half the threads, and the telescope then quickly pointed to the reflected image, by means of an index-level previously set for this purpose.

The method of determining the zero point of an altitude circle, by reflection of the cross-threads of the telescope from the surface of mercury, is erroneously ascribed in page 108 to Benzenberg. Astronomy is indebted to Bohnenberger for this beautiful and accurate process, by which the telescope is "made its own collimator."

Still more strange is the manner in which the author entirely omits any mention of the name of Thomas Godfrey, of Philadelphia, "the inventor," as Dr. Franklin said long since, "of what is called Hadley's sextant." In the American Magazine for the months of July and August, 1758, and in the Notes to the first volume of Dr. Miller's "Retrospect of the Eighteenth Century," are a series of letters which prove, beyond the possibility of doubt, the independent invention of the instrument by Godfrey in 1730, prior to any publication by Hadley upon the subject. It is there shown how the knowledge of the invention could have reached Mr. Hadley; and letters are published, written by both Logan and Godfrey to Dr. Halley, at that time Astronomer Royal of England. The date of these letters was 1732. Two years later, Mr. Logan publicly stated that he had transmitted his letter to Halley in May of that year. "I must own," said he, "that I could not but wonder that our good-will was never acknowledged. I did not then, nor do I now, assume any other merit than this in either of Godfrey's instruments. I only wished that the ingenious inventor himself might by some means be taken notice of, in a manner that might be of real advantage to him." All these circumstances of Mr. Logan's complaint were, as Dr. Miller stated in a foot-note, entirely omitted in the account of the matter which appeared in the Philosophical Transactions, "which strengthens the conjecture that justice has not been done to the original inventor." A claim has lately been brought forward for Sir Isaac Newton; and Herschel speaks only of him and Hadley, making not the slightest allusion to Godfrey. He says of the invention (note to p. 115), —

"Newton communicated it to Dr. Halley, who suppressed it. The description of the instrument was found, after the death of Halley, among his papers in Newton's own handwriting, by his executor, who communicated the papers to the Royal Society, twenty-five years after Newton's death, and eleven after the publication of Hadley's invention, which might be, and probably was, independent of any knowledge of Newton's, though Hutton insinuates the contrary."

Newton's death occurred in 1727. It is certain, therefore, as Dr. Patterson showed in his Address before the American Philosophical Society, at their centennial anniversary, that Godfrey could, at any rate, have had no knowledge of the paper. If the principle, that the publisher of an invention is to be regarded as the discoverer, be applied, as it should be, to this case, nothing can conflict with Godfrey's claim. Allowing all that is asserted concerning the manuscript among Halley's papers, it must have been a mere accident that it was ever found. A great number of interesting and valuable manuscripts of Newton still exist. Are they ever to be published? or will the narrow bigotry, which has thus far withheld them from the world, consign them to oblivion?

Notwithstanding this total neglect of Godfrey's claims, and of the authority of Franklin, Logan, and Patterson, we are yet gratified to perceive in the work indications of a more liberal spirit toward foreign science, than has usually characterized English popular works. Especially with regard to this country, the petty pseudo-nationality which has so long ignored the advances in science made by Americans, is decidedly decreasing. It is perhaps in a less degree evident in the work before us, than in any English astronomical treatise which has been published. The author, although more deeply imbued with the predilections and tastes of his own country than with the liberality which pervades all departments of learning in the home of his ancestors, is too high-minded and noble to allow himself intentionally to misrepresent any facts or theories. When, therefore, — as with regard to Peirce's article on the comet of 1843, to the claims of Godfrey to the invention of the sextant, to the elaborate researches made in America on the theory of Neptune, and many other subjects, — the labors of American astronomers have been passed over in silence, or met with sneers instead of arguments, it is perhaps unjust not to suppose that the author was either ignorant of them, or misunderstood their true bearing.

We have no particular desire to lay stress upon this. It is a very small matter. America, "thanks to God and to herself," needs no foreign praise, no adventitious renown. We are considering solely the merits of the "Outlines of Astronomy."

On page 172 we read that "it is a fact not a little interesting to Englishmen, and, combined with our insular station in that great highway of nations, the Atlantic, not a little explanatory of our commercial eminence, that London occupies nearly the centre of the terrestrial hemisphere"; and in a note the author states, that this central point falls almost exactly upon the town of Falmouth. Ritter called attention to the fact that the continent of Europe occupied this central position. To attempt to define it precisely is futile, and would show a misapprehension of the theories of physical geography, which are large generalizations, in which precise computation is not only uncalled for, but incongruous, and therefore inadmissible. The position of the centre of the terrestrial hemisphere depends of course upon the equator which we assume, and this may be considerably varied without any sacrifice of accuracy. Different individuals would unquestionably estimate it differently. We think, however, that any one who will set a twelve-inch globe in such a position as to bring the greatest possible amount of land above the wooden circle which represents the horizon, will find that the region between Rome and Palermo occupies the highest point. If, on the other hand, we take the view of centrality suggested by Professor Guyot, in his beautiful lectures on "Earth and Man," and select the point from which the three great continental formations diverge at mutual angles of about 120°, we come nearly upon Syria, the supposed cradle of the human race.

A strange assumption is to be found throughout the work. It is, that the heating power of the solar rays is independent of the atmosphere through which they are transmitted. According to this, the summits of mountains should at noon be the warmest places. While refraining from expressing any decided opinion of our own, we cannot but consider it strange that Sir John Herschel should express his own views so dogmatically upon a question where the scientific world are divided, even were his own opinions those of the majority. On page 235 he enters into an argument to prove that the temperature of the sun's surface is higher "than any artificial heat produced in our furnaces, or by chemical or galvanic processes." In favor of this hypothesis he adduces three distinct arguments : "1st. From the law of decrease of radiant heat and light, which, being inversely as the squares of the distances, it follows that the heat received on a given area exposed at the distance of the earth, and on an equal area at the visible surface of the sun, must be in the proportion of the area of the sky occupied by the sun's apparent disc to the whole hemisphere, or as 1 to about 300000. A far less intensity of solar radiation, collected in the focus of a burning-glass, suffices to dissipate gold and platina in vapor. 2dly. From the facility with which the calorific rays of the sun traverse glass, a property which is found to belong to the heat of artificial fires in the direct proportion of their intensity. 3dly. From the fact that the most vivid flames disappear, and the most intensely ignited solids appear only as black spots on the disc of the sun, when held between it and the eye. From the last remark it follows, that the body of the sun, however dark it may appear when seen through its spots, *may*, nevertheless, be in a state of most intense ignition. It does not, however, follow of necessity that it *must* be so. The contrary is, at least, physically possible."

Of these reasons, we will only say that the first and second rest entirely upon the assumption above referred to, and that the third proves only the sun's intense splendor, not its intense heat.

The flame-like protuberances on the eclipsed disc which accompanied the solar eclipse of July, 1842, and which have been since so often described and commented upon, the author considers clearly proved to have been cloudy masses in the solar atmosphere (p. 235) ; and the somewhat similar phenomenon of patches of red light on the edge of the moon's disc, which have been so often observed in lunar eclipses, is ingeniously explained by supposing the rays of the sun, refracted round the earth, to be partially transmitted and partially intercepted by terrestrial clouds, and red light to be thus thrown into the umbra.

There is a widely disseminated notion that the author's father, the illustrious William Herschel, believed in the dependence of meteorological phenomena, especially the state of the weather, upon the phases of the moon. The most careful study of barometric and thermometric records has uniformly failed to indicate any connection between these so widely different phenomena, and no theory has pointed out any reason for such connection; yet the belief in this dependence of the weather upon the moon is still deeply rooted in the minds of many, and defended by citing the great name of Sir William Herschel. Sir John, at last, by a letter published in Schumacher's Astronomical Journal, openly denied that either his father or himself had entertained such views, or pretended to be able to predict, by any length of time, the state of the weather. In the work before us, he again states his belief that there is no evidence of any influence of the moon upon the weather, excepting the tendency of clouds to disappear under the full moon, — a tendency which he has independently observed, but to which Humboldt alluded in his personal narrative as a fact known to the sailors of Spanish America.

After a full account of the two most conspicuous celestial bodies, the sun and moon, of the theory of eclipses and the law of gravitation, the author proceeds to the consideration of the other members of our solar system, and devotes the three remaining chapters of the first part of his book to the planets, satellites, and comets. We have some strictures to make upon these chapters, although we must acknowledge not having studied them enough to appreciate their merit; — perhaps in consequence of the impressions derived from the first perusal.

The small planets, belonging to the extensive and remarkable group between Mars and Jupiter, have, by the common consent of astronomers, received the name of *asteroids*. This term was originally proposed by the elder Herschel, and though perhaps open to criticism, has been so universally adopted, that it must now be regarded as their legitimate name. The word *asteroid* is fortunately in the index, but is to be found, we believe, in no other part of the book, excepting as a definition on page 294. The name *ultra-zodiacal planets* has been substituted, and, with a single exception, used throughout the volume. The degree of correctness of this term may be inferred from the fact, that, out of the ten planets known to belong to this group, there are only four which ever pass the limits of the zodiac, the other six being as strictly confined within these limits as any of the large planets. On page 426, the asteroids are called *extra-tropical planets.* We are at a loss to know what this means. The only interpretation which we can give to the word *extra-tropical* is "outside the tropics"; but we cannot believe that so experienced an observer as Sir John Herschel would deny that every planet comes, nearly once a year, within this category.

While speaking of the "ultra-zodiacal" or "extra-tropical" group, the author alludes to the empirical formula which has been called "Bode's law." In the "Treatise on Astronomy," the author stated his conviction that "the circumstances mentioned lead to a strong belief that it is something beyond a mere accidental coincidence, and belongs to the essential structure of the system." In the present edition, the sentence is retained, with the exception that the verbs have been changed from the present to the imperfect tense.

In the note to the new work, he says: —

"The empirical law itself, as we have above stated it, is ascribed by Voiron, not to Bode, (who would appear, however, at all events, to have first drawn attention to this interpretation of its interruption,) but to Professor Titius, of Wittemberg. (Voiron, Supplement to Bailly.)"

Bode was neither the first to draw attention to the empirical law, nor to its interruption; and had the author looked a little farther, he would have found that Voiron, who merely copied the reference to Titius, was not the only one who had called attention to his claim. Lalande mentions, in the appendix to his Bibliography, (p. 545,) that Titius, in the notes to his translation * of Bonnet's "*Contemplation de la Nature*," published in 1772, remarked that the distances of the planetary orbits from the orbit of Mercury might be represented by the multiples of 3, but that a term of the series was wanting between Mars and Jupiter, where an unknown planet might perhaps exist, and thus fill the gap concerning which Kepler had speculated so much.

---

* See edition of 1783, p. 14, where, however, a point is erroneously printed throughout instead of the sign of addition.

Biot, too, in his series of articles in the "*Journal des Savants*, 1846," not only alluded to Titius in this connection, (as did also Gauss in the "*Monatl. Correspondenz*," 1802,) but gave the reference to Lalande. Bode first mentioned it in his "*Einleitung zur Kenntniss des gestiruten Himmels*," referring, however, to two articles by Wurm, in the Berlin Astronomical Almanac for 1790 and 1791. In these two papers, Wurm had given the formula, apparently without knowing that it had been previously published by Titius, and in a general algebraical form, which applied also to the distances of satellites from their primaries.* In communicating this formula, together with several other equally curious ones, Wurm had the merit of calling especial attention to the fact that the harmony of the progression was broken by Mercury. The proposition was, however, stated in such a form as to be approximately true, by reckoning the distances, not from the sun, but from the orbit of Mercury. Wurm did not pretend to believe the progression to be anything more than a curious coincidence, and earnestly requested that too much weight might not be attributed to it. He called the idea an astronomical fantasy, (*astronomische Schwärmerei*,) and alluded to the analogy which Kepler had discovered between the five regular solids and the five planetary intervals. This analogy, which was subsequently destroyed by the discovery of Uranus, represented in fact all the planetary distances quite as well as the formula of which we now speak. Kepler announced it, in triumphant language, in his "*Mysterium Cosmographicum*," a work written expressly to develop this theory: —

"Quid mundus, quæ causa Deo, ratioque creandi,
Unde Deo numeri, quæ tantæ regula moli,
Quid faciat sex circuitus, quo quælibet orbe,
Intervalla cadant, cur tanto Jupiter et Mars,
Orbibus haud primis, interstinguantur hiatu:
Hic te Pythagoras docet omnia quinque figuris.
Scilicet exemplo docuit, nos posse renasci,
Bis mille erratis, dum fit Copernicus, annis,
Hoc, melior Mundi speculator, nominis. At tu
Glandibus inventas noli postponere fruges."

We are, then, only warranted in considering the formula of Titius, Wurm, or Bode, as the reader may please to call

* Prof. Challis, of Cambridge, Eng., published an interesting paper in the Transactions of the Philosophical Society of that University, (vol. iii. p. 171,) to show that "Bode's law" finds application in the systems of satellites as well as in the system of primary planets. He was evidently unaware of this article by Wurm.

it, as a neat representation of the planetary distances, valuable for the mnemonic aid which it affords. The illustrious Gauss has repeatedly protested against its being termed a law, inasmuch as it is, at the best, but approximate, and in no respect possessing the precision which characterizes Nature's laws. The discovery of Neptune at the distance 11 beyond the orbit of Uranus, while the formula would make this distance 19, has, we conceive, shaken the faith of the firmest adherent. While for this and many other reasons we differ decidedly from the author in his views regarding the discovery of Neptune, we cannot but admire the exquisite applicability of his quotation from Schiller's epigram on Columbus, and abstain from saying anything which could mar the beauty of the thought : —

> " Mit dem Genius steht die Natur in ewigem Bunde,
> Was der Eine verspricht, leistet die Andre gewiss."

> Nature is bound in a never-ceasing alliance with genius,
> That which is promised by one, ever the other provides.

In consequence of the confusion which arose in the nomenclature of the satellites of Saturn, from the circumstance that the order of their discovery was not that of their distances, the author proposed, some time since, a mythological nomenclature, analogous to that of the planets. The unnecessary multiplication of empirical names should unquestionably be avoided ; but in this case the new nomenclature, though unwieldy, would perhaps tend to perspicuity, and it seems, therefore, to have been adopted by Messrs. Bond and Lassell, each of whom, after his independent detection of an eighth satellite, gave to the new body the name *Hyperion.* This is not mentioned in the text of the work before us, which was probably printed at the time. The author, desirous of facilitating the remembrance of the names which he had proposed, suggests (in the note to p. 337) the following pentameters as affording an easy artificial memory, the series commencing with the most distant satellite : —

> " Iapetus, Titan, Rhea, Dione, Tethys [pron. Tĕthys]
> Enceladus, Mimas ——— "

The name selected for the new satellite, however appropriate it may be, does not seem inclined to lend itself to verse, nor, by its interpolation, to improve the rhythm. But after placing it in its proper position in the line, the names may be read into a kind of anti-Virgilian hexameter, which may be of service to those who desire to remember them, and are accustomed to rely upon mnemonic aid : —

Iapetus, Hyperion, Titan, Rhea, Dione,
Tethys, Enceladusque, Mimas, — Titanides octo.

Although availing ourselves of the "poetic license" to its full extent, we are thus enabled to give the correct quantity to the first syllables of Tethys and Mimas.

We are told on page 322 that Neptune is attended "very probably by two satellites, though the existence of the second can hardly yet be considered as quite demonstrated."

In the chapter upon comets, a great number of the errors which existed in the former edition have been corrected. But the greater part of the chapter consists of new matter. We shall not stop to criticize the statement that some comets move in hyperbolas, although Professor Peirce has shown the extreme improbability of this. No one will deny the *possibility* that an intense perturbation by one of the large planets might, under peculiar circumstances, throw a comet into a hyperbolic orbit ; so, too, the centre of gravity of the solar system might be in a direction sufficiently different from that of the sun to cause an elliptic orbit to *appear* hyperbolic ; but the eccentricity of none of the orbits, to which Sir John Herschel refers, can be said to differ sufficiently from unity to put their hyperbolism beyond question.

A highly interesting account of Halley's and of Biela's comet is given, in the course of which the author states his views concerning the formation of comets' tails. While we admire the clearness with which these views are expressed, we would take the same ground as we took before with regard to the solar heat, — that while different astronomers are so far from agreeing in their opinions, this want of unanimity ought to be alluded to in an elementary work. The near approach of Biela's comet to the earth's orbit is mentioned, and the remark made, that had the earth, "at the time of the comet's passage in 1832, been a month in advance of its actual place, it would have passed through the comet, — a singular rencontre, perhaps not unattended with danger." *

In the account of the periodicity of Faye's comet, no allusion whatever is made to Professor Goldschmidt, who first discovered that it moved in an ellipse of short period.

Respecting the periodic comet discovered by Peters in 1846, the author says, that elliptic elements have been com-

---

* *Qu.* To the earth, or to the comet?

puted by D'Arrest, "which go to assign it a place among the comets of short period, viz. 5804.3, days, or very nearly 16 years. The eccentricity of the orbit is 0.75672, its semi-axis 6.32066, and the inclination of its plane to that of the ecliptic 31° 2′ 14″." It is most true, that D'Arrest computed these elements ; but Sir John Herschel does not seem to have been aware that Dr. Peters afterwards published, in 1847, — more than eighteen months before the publication of the "Outlines of Astronomy," — a labored and classic work upon this comet, — the "*Memoria sopra la Nuova Cometa Periodica di tredici anni*," — in which, after a thorough discussion of the whole series of observations from June 26th to July 21st, he deduces a final orbit. The resulting period is about 12$\frac{17}{20}$ years, or less than 4700 days. The eccentricity of the orbit is 0.72134, its semi-axis (major) 5.48558, and the inclination of its plane to the ecliptic 30° 24′ 24″. Peters has still farther shown that the period cannot be so long as fourteen years.

The great comet of 1843 is discussed at length, but the author does not appear to have seen Peirce's important article in the American Almanac for 1844, which is by far the most thorough research concerning that comet ever published. Nor is any mention made of the remarkable observation on the 27th February, the day of the perihelion passage, by Captain Ray, and given to the public by Hon. William Mitchell, of Nantucket, although this is the observation which has furnished the chief difficulty to computers ; nor yet a word said of the extremely valuable observations, made on the 28th, by Mr. Bowring, in Chihuahua, where the comet was visible from nine o'clock in the morning until sunset. These observations were published both in the *Comptes Rendus* of the French Academy, and in Schumacher's Journal. And what is very strange, while we read on page 370 that "there seem good grounds for believing that its whole course cannot be reconciled with a parabolic orbit, and that it really describes an ellipse," yet not one of the five orbits given as "those which seem entitled to most confidence" is an ellipse. Herschel has, moreover, given three out of these five orbits erroneously. The first is a hyperbola, computed as an experiment by Encke, before the series of observations was complete, — and an orbit to which the Prussian Astronomer Royal would attach but little weight, as it deviates from Clarke's observation of Feb. 28th by nearly seven minutes.

Herschel has stated the Greenwich time of the perihelion passage in this orbit to be, Feb. 27.45096. The fraction should, according to Encke, be .46056. In Plantamour's elements, he has given the inclination as 35° 8′ 56″; it should be 35° 45′ 39″. In the third orbit, the time of perihelion is put down, Feb. 27.39638, and the longitude of the node, 1° 48′ 3″; these numbers should be 27.42700 and 1° 48′ 43″.

But we are throwing away our time and the space allowed us, by dwelling upon errors of minor importance. On the next page we read that the heat to which the comet was subjected surpassed, in the proportion of 24½ to 1, that in the focus of a certain great lens, which melted agate and rock crystal.

After mentioning the marked similarity of the orbit to that of the comet of 1668, whose identity with this one may be considered as almost demonstrated, Herschel proceeds to state the arguments in favor of its identity with that of 1689, and its consequent period of 21⅞ years. Walker first suggested this period, but no allusion is made to him. Peirce recalculated the elements of the latter comet and found an orbit differing much from Pingré's, and sufficiently similar to that of the present comet to offer no obstacle to the hypothesis. He rejected the theory, however, because he found it incapable of representing the observations. Herschel, on the other hand, does not look at the question from this point of view, but says (p. 372), —

"It is worth remarking, that this period, calculated backwards from 1843.156 will bring us upon a series of years remarkable for the appearance of great comets, many of which, as well as the imperfect descriptions we have of their appearance and situation in the heavens, offer at least no obvious contradiction to the supposition of their identity with this. Besides those already mentioned as indicated by the period of 175 years, we may specify as probable or possible intermediate returns, those of the comets of 1733?, 1689 above mentioned, 1559?, 1537, 1515, 1471, 1426, 1405-6, 1383, 1361, 1340, 1296, 1274, 1230, 1208, 1098, 1056, 1034, 1012, 990?, 925?, 858??, 684, 552, 530, 421, 245 or 247, 180, 158. Should this view of the subject be the true one, we may expect its return about the end of 1864 or beginning of 1865, in which event it will be observable in the Southern Hemisphere, both before and after its perihelion passage."

It would hardly be difficult, we may be permitted to say, to furnish for any theory a list of corresponding years, in which somebody has reported a comet. The catalogue of comets, real or imagined, is so large, that, in many cases, some record may be found of sixty or seventy during the lapse of a century. It cannot, of course, be inferred that so many have actually appeared ; for, owing to mistakes of date, and to the assumption that every brilliant meteor was a comet, the true number has been unquestionably much exaggerated. Whenever a monarch died, or any calamity occurred, whether a comet had been seen or not, it was yet inferred that one must have been in the heavens ; inasmuch as celestial portents always preceded such occurrences, and the historian or the biographer of royalty seldom omitted to record the fiery swords in the heavens. These become, in course of time, comets. The translation and publication, by Goubil, Guignes, and latterly by Edouard Biot, of the Chinese astronomical annals, has furnished a large addition to the catalogues previously existing, and we are thus enabled to find, within a year of almost any given date, some recorded appearance.

When nothing is said concerning a comet excepting that it was seen, we are of course unable to adduce any argument for or against the hypothesis of its identity with another one ; but fortunately, there is, in most cases, some little remark appended, containing either a rough intimation of the part of the heavens where it appeared, or of the time at which it was visible. It is so with regard to the most of those cited above.

The perihelion distance of the comet of 1843 is, as the author has already said, smaller than that of any other comet which has been recorded. The angle between node and perihelion is about 82°, and it is therefore evident that only an extremely small part of the orbit can be situated north of the ecliptic, — a part which the comet would require about two hours to traverse. No comet, therefore, which has been observed to be in north latitude, except on the day of its perihelion, can be for a moment presumed to be identical with this one. From December to July, it can never have been seen in the signs between the middle of Cancer and Sagittarius, nor in the other months between Capricorn and Gemini. This is clear to any one who will reflect for a moment, or draw the roughest diagram. And, as the axis of the orbit is nearly perpendicular to the line of nodes, and the orbit very eccentric, the comet is invisible to all observers north of the equator, except for a very short time, directly before and after the perihelion passage.

Let us now compare the comets of the author's list with the comet of 1843, using for reference the Cometography of Pingré, from which, as Sir John Herschel states, " all these recorded appearances are taken." We will consider them separately.

1733. Of this one, Herschel himself says that it " seems too early in the year."

† 1559. This year two comets are mentioned, — the one was seen in the east for three or four weeks in May and June, which never could have been true for the comet of 1843 ; of the other we have only the record, " Comet in November."

1537. " Comet in January," says Pingré, " in Pisces, and another in May in Taurus. These are manifestly the two following ones,".— those of 1538 and 1539. Be this as it may, neither can agree with the supposition of identity, for the first was seen in 17°, the second in 12°, north latitude.

1515. A comet is said to have announced the death of Ferdinand the Catholic, who died in 1516, the year given by Pingré, and not in 1515, as Herschel asserts.

1471. The first one mentioned this year was seen *in Virgo in the month of March.* The second was the comet known as that of 1472. It first appeared in 1471, and *lasted many months.* Its orbit, moreover, has been calculated by Halley and Langier, and found totally different from that of the comet of 1843.

† 1426. " On the 9th of June," says Pingré, " a comet was seen above the church of the Frères-mineurs ; it extended its rays towards the great square of the town (Liege) ; it lasted a week." As we have no means of knowing the position of the observer, we can say nothing on the subject, and leave to the author all the support for his theory which he can deduce from the comet of 1426.

† 1405 – 6. No comet is recorded in 1405. " In the first half of 1406, a comet was seen in the west."

1383. The following is Pingré's account : — " In 1383, the tomb of St. Dominique was opened, and, as long as it remained open, a large and very brilliant star, from which three tails diverged, *remained immovably fixed* above the church of this saint. I do not consider this phenomenon to have been a comet."

1361. Pingré says that the mention of a comet in this year was manifestly through mistake, and that the first one of

1362 was intended. This was too far north, having been near $\lambda$ Pegasi. It was there, moreover, *in the month of March*, and visible *for two months.* The second comet of 1362 was so far north that it did not set. Its rays are said to have been a foot long.

1340. "Evidently a southern comet and a very probable appearance." — *Herschel.* "Seen *at the end of Virgo or beginning of Libra*, toward the last of March." — *Pingré.*

1296. "A comet, *visible for a long time* in the heavens, announced future events, and especially the death of the Emperor Adolphus, *who died in* 1298."

† 1274. "Probably a return of that of 1661."

† 1230. No particulars known. "Perhaps a return of Halley's comet." — *Pingré* and *Herschel.*

The comet of 1231 was seen *in Scorpio* in March, and had at one time a north latitude of 60°.

1208. Pingré believes this to have been the planet Venus.

† 1098. "The very night of the taking of Antioch, (June 3,) the comet, which is accustomed to announce the revolution of empires, shone out among the stars of the sky, and spread far the brilliancy of its rays. A redness of fire was also seen between the north and east." Nothing more is known of it.

1056. This comet was so far to the northward as to be "among the stars which never set."

† 1034. With regard to this comet we know nothing which would make the supposition of identity preposterous.

1012. Was seen *for three months.*

† 990. "'Comète fort épouvantable' some year between 989 and 998." — *Herschel.* Pingré mentions *nine* within this period. Seven of them could not possibly have been identical with the comet of 1843. Of the other two nothing is known, but that they are said to have appeared; this, however, upon very slight authority.

925. This one was seen in the autumn after sunset, which sets the hypothesis of identity at rest.

858. At the death of Pope Benedict III., in April, a comet was seen in the east, with its tail toward the west. That of 1843 could not have been in this position.

684. Three comets are mentioned this year. "Dates begin to be obscure," says the author! It is nevertheless impossible for either of the three to be brought into conformity with his hypothesis, for the first was seen *in September*, to-

*wards the west*, the second between Christmas and Epiphany, *near the Pleiades*, and the third was visible *for three months.*

† 552. "Torches were seen in heaven, and a comet appeared, the year before the death of Theodebald."— *Pingré.*

530 – 531. The first was seen *for twenty days* in the west, with *its tail towards the zenith.* The second went from Arcturus to the Great Bear, far to the north.

† 421. Kao-tsou ascended the throne of China in 420 or 421. "In the fourth moon of the first year of his reign a comet appeared." "In Europe, an admirable sign was seen in the heavens. Could it have been a comet ?"— *Pingré.*

† 245 or 247. For the first of these the hypothesis is not impossible. The second was seen *for* 156 *days.*

180. This one was *near Sirius*, in November.

158. "Janssen Twisk, in his Treatise on Comets, mentions one, *qui a dû paraître cette année !*"— *Pingré.*

We have thus reviewed the whole list, excepting the comet of 1689, the similarity of whose true elements was shown at the time by Professor Peirce's investigations, which have furnished Herschel with his arguments as respects this comet. We see, that, of twenty-eight recorded comets, which are specified by Sir John Herschel "as probable or possible intermediate returns," there are seventeen which could not possibly, under any supposition, be made to accord with his theory. Of the eleven which remain, Pingré doubts the existence of *four*, *two* correspond with probable appearances of other periodical comets, and of *three* others nothing whatever is known.

Had so loose and unwarranted assertions appeared in any elementary work made by a professed compiler, they would deserve and receive the severest reprehension. In a work like the one before us, and coming from such an author, they cannot fail to excite deep regret,— and the deeper, the greater the author's reputation, and the injury which they are therefore likely to do. Why was the concluding sentence of this chapter in the "Treatise on Astronomy" omitted in the present edition ?

In passing to the criticism of the other divisions of the work, we will state that the inaccuracies to which we have alluded have not been detected by any search instituted with this object, but are those which struck our attention upon the first cursory perusal. The work abounds also in mistakes of carelessness, to which we have not thought it necessary to allude, because they are not particularly dangerous, since the slightest reference to authorities would rectify them. Thus, for instance, the author says (p. 351) that Halley's comet in 1835 was "observed at Pulkowa up to the very day of its perihelion passage." He meant unquestionably Dorpat, since the Pulkowa Observatory was not established till 1839. On page 356, reference is made to Schumacher's Catalogue of Comets. The celebrated catalogue by Olbers is intended, which was published in the collection of astronomical papers edited by Professor Schumacher. So, too, we find (p. 159) that "the differences of longitude between the *observatories of New York*, Washington, and Philadelphia, have been very recently determined [by electro-magnetic telegraph] by the astronomers at those observatories."

The second part of the book treats, as has been already said, of the lunar and planetary perturbations. It is this part of the work to which Herschel has devoted the most attention in preparing the "Outlines." But, as he justly remarked in the Preface, this subject cannot be made elementary. The author has succeeded, better than would perhaps have been supposed, in expressing a number of important theorems in ordinary language, and in giving a general sketch of the subject without using the phraseology of the calculus. But we much doubt whether his mode of presenting the subject will prove attractive to any class of students. We think that by far the majority of readers will pass over these chapters; that those who possess sufficient mathematical taste to relish the account here given will need no aid of the kind in order to comprehend the analytical treatment of the subject; and that mathematicians who are already familiar with the theory of perturbations will find Herschel's development heavy and yet diffuse. Still we are ready to acknowledge that the difficulty lies rather in the nature of the problem than in the author.

The theory of Neptune is the only part of which we intend to speak, and we desire the more earnestly to speak of this, not so much on account of our conviction of the untenable nature of the ground here taken, and of the flaws in the reasoning, — flaws none the less perceptible from the labor bestowed on the endeavor to conceal them, — as on account of the authority which the author's name carries with it, Credence would unquestionably be given to his statements, were they not boldly challenged and clearly refuted. Men have even been found in this community ready to consider a slur in the "Outlines of Astronomy" a sufficient offset to the authority of America's most illustrious geometer.

It almost seems as if the very name Neptune were, throughout the book, under the ban of some evil genius; for seldom indeed does it occur, unaccompanied by an erroneous statement. The first place in which it is to be found, in this division of the work, is on page 427, where it is stated that "forty-one revolutions of Neptune are nearly equal to eighty-one of Uranus, giving rise to an inequality, having 6805 years for its period." The author probably obtained these numbers by using the incorrect elements of Uranus which he has given in his appendix; — elements, once a fair approximation, but utterly inadequate to furnish data of proper accuracy, since the careful determination of the orbit of Uranus by Le Verrier. Successive approximations to the ratio of the two periods are $\frac{24}{47}$, $\frac{25}{49}$, and $\frac{49}{96}$, the last being correct to the fifth decimal place. The period of the inequality cannot differ much from 4051.26 years.

The circumstances which preceded and accompanied the discovery of Neptune are known to the public. So, too, is the discussion which arose between the partisans of the two candidates, as well as the subsequent and still more remarkable discovery of Professor Peirce, that the problem, as it had presented itself to Messrs. Le Verrier and Adams, admitted of two solutions, of which these geometers, relying on "Bode's law," selected the wrong one. All this is matter of history, — we have only to do with the assertions in Herschel's book. Sir John denies the fact that the solution of Le Verrier and Adams is not the correct one, and endeavours to show that the uncertainty of the calculations was so great that Neptune may be considered as coming within Adams's theory. The course which he has taken to prove this is such, that those who assent to his views can only allow to Adams the merit of having approximately found the period of Uranus's greatest perturbation, and assumed that it was at that time in conjunction with the disturbing planet. We deliberately assert that the position which Herschel has taken would, when legitimately carried out, deprive Mr. Adams of any other claim to having made a brilliant investigation, than that to which a man would be fairly entitled, who, after computing the epochs of Uranus's greatest variation from theory, should have inferred the position of Neptune from a graphical approximation. This planet would, if we use Herschel's diagram, (Plate A, fig. 4,) be in conjunction with Uranus at the time when, in the curve representing the residual differences between observation and computation, the great wave should cut the "medial line." On the other hand, the merit which American astronomers accord to Le Verrier and to Adams is scarcely, if at all, inferior to that which would have been attributed to these geometers had Neptune been the planet of their theory.

We take the liberty to quote a sentence from Sir John Herschel's "Results of Astronomical Observations made at the Cape of Good Hope," published in 1847. The author is speaking of a double star, ($\gamma$ Virginis,) whose period he had formerly computed as 628.9 years, but has since concluded to be but 182.1. He shows that the observations, on which his former orbit was founded, may be represented by either of the two; and adds, —

"This is not the first, by many instances in the history of scientific progress, where, of two possible courses, each at the moment equally plausible, the wrong has been chosen." — p. 294, §191.

Mr. Adams seems indeed to have, in Sir John Herschel, a most maladroit champion. The first assertion which the author makes, in approaching the history of the investigations which led to the discovery of Neptune, is (p. 507), "that up to the year 1804, it might have been safely asserted that positively no ground whatever existed for suspecting any disturbing influence." Neptune had, it is true, not been in conjunction with Uranus since about 1651, and the earliest observation of the latter which we possess was made in 1690; but, according to Mr. Adams's theory, the two planets would have been in conjunction just at the time of that observation. (See also page 517.)

Still worse is the remark, on the same page, that "the idea of setting out from the observed anomalous deviations, and employing them as data to ascertain the distance and situation of the unknown body, appears to have occurred only to two mathematicians, Mr. Adams in England, and M. Le Verrier in France, with sufficient distinctness and hopefulness of success to induce them to attempt its solution." In the London Athenæum for October 3d, 1846, will be found a letter from Sir John Herschel to the editor, dated October 1st, the day after the news of the discovery of Neptune at Berlin had reached him. We quote from this letter :

"On the 12th July, 1842, the late illustrious astronomer Bessel honored me with a visit at my present residence. On the evening of that day, conversing on the great work of the planetary reductions, undertaken by the Astronomer Royal, — then in progress and since published, — M. Bessel remarked, that the motions of Uranus, as he had satisfied himself by careful examination of the recorded observations, could not be accounted for by the perturbations of the known planets ; and that the deviations far exceeded any possible limits of error of observation. In reply to the question, Whether the deviations in question might not be due to the action of an unknown planet, he said that he considered it highly probable that such was the case, — being systematic, and such as might be produced by an exterior planet. I then inquired whether he had attempted, from the indications afforded by these perturbations, to discover the position of the unknown body, — in order that 'a hue and cry' might be raised for it. From his reply, the words of which I do not call to mind, I collected that he had not then gone into that inquiry ; but proposed to do so, having now completed certain works which had occupied too much of his time. And, accordingly, in a letter which I received from him, after his return to Königsberg, dated November 14, 1842, he says, 'In reference to our conversation at Collingwood, I *announce* to you (*melde ich Ihnen*) that Uranus is not forgotten.'"

Bessel spoke of his investigations in a public lecture delivered in Königsberg, February 28th, 1840, and published in 1847 ; but an attack of severe illness, which terminated in death, prevented him from carrying out his computations.

On page 510, stress is laid upon the fact that Mr. Adams stated that the errors since 1840 might be much diminished by taking a smaller semi-axis, "and that a mean distance of 33.3 would probably satisfy all the phenomena very nearly." This hasty conclusion was obtained by an application of the "rule of three" to the discrepancies of the elements since 1840, and we doubt whether Mr. Adams will be grateful to the author for dwelling upon it. Apart from the fact, that this distinguished mathematician availed himself of more refined methods for determining the orbit, it is now known that an impassable barrier to inferences of this kind exists at the mean distance of 35.3. An exceedingly important change in the character of the perturbations takes place at this point, — a change so great, that investigations made with regard to the region on one side cannot be extended to the other. The discordances would be increased, not diminished, by a decrease of the mean distance from 36 to 35.

Continuing the strange course, which he has hitherto pursued with regard to the claims of Messrs. Le Verrier and Adams, Herschel urges the plea, that the hypothetical elements not only place the planet, at the time of its discovery, in a *direction* extremely near that of Neptune, but also at a *distance* "very much more approximately correct, than the mean distances of the respective orbits." This is true, Neptune having been at his aphelion nearly at the time when the theoretical planet would have been in perihelion, — the enormous eccentricity attributed to the orbit of the latter producing a great influence in decreasing the perihelion distance. But we can scarcely consider this as strong ground in favor of Herschel's position. The two mathematicians, who solved the "inverse problem of the perturbations of Uranus," did not profess to solve it for any particular epoch, but attempted to find the true orbit and mass of the disturbing planet; the elements, when known, would enable us to assign its direction and distance at any moment. Herschel has given a table of comparison, which extends, however, over but few years on each side of the conjunction. The following one covers more ground, extending through one revolution : —

| | True Longitudes of | | | True Distances of | | |
|---|---|---|---|---|---|---|
| Year. | Neptune. | Le Verrier's planet. | Adams's planet. | Neptune. | Le Verrier's planet. | Adams's planet. |
| | ° | ° | ° | | | |
| 1680 | 320.0 | 50.7 | 67.8 | 30.01 | 38.25 | 39.93 |
| 1700 | 4.4 | 79.2 | 94.5 | 29.84 | 39.63 | 41.30 |
| 1720 | 49.1 | 106.5 | 119.8 | 29.77 | 40.04 | 41.74 |
| 1740 | 93.8 | 134.0 | 145.5 | 29.86 | 39.40 | 41.09 |
| 1760 | 138.1 | 163.0 | 172.6 | 30.04 | 37.84 | 39.46 |
| 1780 | 181.8 | 195.1 | 202.7 | 30.22 | 35.68 | 37.15 |
| 1800 | 225.9 | 231.4 | 236.8 | 30.30 | 33.57 | 34.74 |
| 1820 | 268.5 | 270.9 | 275.2 | 30.23 | 32.64 | 33.06 |
| 1840 | 312.0 | 312.0 | 315.9 | 30.02 | 32.63 | 32.91 |
| 1860 | 356.4 | 351.0 | 355.0 | 29.87 | 34.26 | 34.37 |
| 1880 | 41.1 | 25.8 | 29.9 | 29.77 | 36.48 | 36.73 |

In the note to page 517, the assertion of Professor Peirce, that the coincidence in *direction* between Neptune and the planet of Le Verrier's theory was the result of a "happy accident," is said "to be founded on a total misconception of the nature of the problem." If we understand the matter at all, Professor Peirce took the problem, as Le Verrier and

Adams propounded it, without making any assumption as to its nature. But this note is unworthy to be dwelt on.

The chapter closes with a statement so diametrically opposed to the truth, that we have hesitated considerably before deciding to mention it. But though it carries the evident marks of its untruth on its very face, yet these might pass unnoticed by persons not versed in astronomy. We therefore allude to it, premising that the formulas and numerical data alluded to were not computed by Mr. Walker, as the author states, but by Professor Peirce. This geometer gave the following table of comparison between the perturbations of the longitude of Uranus, which would be produced by Adams's hypothetical planet, and those which are really produced by Neptune: —

| | Action upon the longitude of Uranus by | | | Action upon the longitude of Uranus by | |
|---|---|---|---|---|---|
| Date. | Adams's planet. | Neptune. | Date. | Adams's planet. | Neptune. |
| 1840 | −118″ | −3377″ | 1797 | +163″ | −1816″ |
| 1835 | − 96 | −3235 | 1792 | +181 | −1967 |
| 1829 | − 70 | −2964 | 1787 | +178 | −2210 |
| 1824 | − 44 | −2684 | 1782 | +150 | −2504 |
| 1819 | − 13 | −2393 | 1769 | + 21 | −3225 |
| 1813 | + 35 | −2072 | 1756 | −105 | −3431 |
| 1808 | + 83 | −1881 | 1715 | +191 | −1845 |
| 1803 | +123 | −1781 | | | |

This enormous difference is met by Herschel with the greatest composure. He says, — "This is easily explained. Mr. Adams's perturbations are deviations from Bouvard's orbit of Uranus as it stood immediately previous to the late conjunction. Mr. Walker's are the deviations from a mean or undisturbed orbit, freed from the influence of the long inequality resulting from the near commensurability of the motions."

We are at a loss what to say of this extremely cool assertion, excepting that it is without the least shadow of foundation. The table has no reference to any "deviations," nor to any particular orbit of Uranus, but to the perturbative influence exerted upon Uranus by the real and by the hypothetical planet; and the comparison, as given in the table, is perfectly legitimate.

We have consumed the space allotted us in the consideration of the first two parts, which form about three quarters of the work; and are thus debarred from considering at present the remainder of the volume. This is entirely in keeping with the part which we have reviewed, — containing many errors and omissions.

Believing that the "Outlines of Astronomy," supported as they are by the name of Herschel, would be considered as authority, should public attention not be directed to the inaccuracies and incompleteness of the work, we have deemed it our duty to do this. Too much weight is often given, in our country, to a great foreign name ; and we are well aware that criticisms upon a Herschel will not be received with favor, or even with lenity. The duty, therefore, appears to us so much the more imperative. But while endeavoring to expose the errors which pervade the volume, we have striven to speak of the distinguished author with the respect and deference to which his eminent services to science, and his world-wide reputation, entitle him. If the student be on his guard against implicit reliance upon the correctness of the book, he may unquestionably derive from it essential benefit. It is a work of ability, replete with information, and parts of it are well calculated to excite the enthusiasm of those who possess a taste for the study of Nature in her grandest phases. The errors, numerous as we have seen them to be, are ye generally the consequence rather of superficial investigation than of anything worse. The only exception which we are disposed to make is in the account of Neptune ; and we can make allowances for peculiar sensitiveness in Sir John Herschel on this subject. It would perhaps be expecting more than human nature would warrant, were it otherwise. Still, the community has a right, in a didactic work, to demand a narration of facts, rather than an *ex parte* statement. This—however difficult in cases where the narrator has himself played a part—we should yet have expected from the author of the beautiful paragraph in the Introduction to the present work, in which he says that the devotee of science "must strengthen himself by something of an effort, and resolve for the unprejudiced admission of any conclusion, which shall appear to be supported by careful observation and logical argument, even should it prove of a nature adverse to notions he may have previously formed for himself, or taken up, without examination, on the credit of others. Such an effort is the first movement of approach towards that state of mental purity which alone can fit us for a full and steady perception of moral beauty, as well as physical adaptation. It is the 'euphrasy and rue' with which we must 'purge our sight,' before we can receive and contemplate, as they are, the lineaments of truth and nature." B. A. G.